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STOCKHOLM 60

October 16, 1957.

Professor Joshua Lederberg
University of Melbourne
Department of Bacteriology
c/o Professor Sidney D. Rubbo
Carlton N.3, Victoria
Australia

Dear Joshua:

Many thanks for your letter. In the meantime you have probably received my second letter. Since Cavalli has now officially accepted, we have all four main speakers settled (Cavalli, Ephrussi-Taylor, Jacob, and Stocker). There remains the problem of the panel. It seems that the whole symposium will have to be limited to between three and four hours. This means that no more than about 15-30 minutes will be open for panel discussion. Also it appears presently that while the congress will be able to pay the travel and living expenses of the moderators and the invited speakers, there will be probably no possibility to contribute to the expenses of the panel discussers. It would be therefore preferable if the discussers could be selected from among the people who already spontaneously declared their intention to participate at the congress. I could find five names who may belong to this category: Bertani, Mortimer Starr, Zamenhof of Columbia, Szybalski, and Wollman of the Pasteur Institute. Would you consider these persons suitable for the panel? Are there any other ones whom you would consider of special importance? For your information, I am including the list of the people who have declared their intention to participate and have expressed interest in microbiol. genetics. If you find any other names on this list who should be invited as discussers, please let me know. Are there any other persons whose invitation you would consider of prime importance?

after each paper.

I was very much interested in your work on mammalian lymphocytes. The test for antibody formation by immobilization of bacteria in micro-droplets is most ingenious. We have several ascitic lymphomas where we suspect that some antibody formation may be occuring but we are far from sure. We are presently engaged in trying to find this out with more certainty. We found that some lymphomas inhibit the growth of sarcomas and carcinomas of unrelated strains when growing together in mixture. Whether this phenomenon is immunological or not we are as yet unable to say. If you would like to test any of these, I would be most happy to send them along. You would need either the C3H or the DBA strain of mice to keep the lymphomas going. If you just want to do short term experiments we can supply you with a sufficient number of donor and recipient animals to keep the tumor going for a month or two.

As to preservation by freezing, we are not using glycerol; in fact we are not adding anything at all. I am including a brief description of our technique. Mühlbock

has recently found that this method is applicable even to a large number of other tumor types than the ones we have tested, among them several endocrine ones. Nevertheless, there were in his material a few benign, differentiated, slowly growing and conditioned endocrine tumors which did not survive the procedure. Whether normal lymphocytes would survive or not I do not know; malignant lymphoblasts survive excellently. Of course it would be probably easy to try and see if you have a good criterion for viability, such as antibody formation. In our experience it is of utmost importance not to add any sort of artificial electrolyte solutions to the tissues or cell suspensions that are being frozen. If you add electrolyte you may still protect the cells with glycerol but in our experience the yield of survival is inferior to direct freezing without any treatment at all and may also possibly lead to some surface changes in the cells.

I was glad to hear that you had an interesting time in Australia. If you are passing through Europe, could you not by any chance stop here for a day ot two? Of course I imagine that your time is probably very limited. If you could do it, however, we would be most delighted.

Wishing you a very pleasant trip and with warm greetings, also to Ester,

yours as ever,

George Klein